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Interactive Comment

Interactive comment on "Increased ocean carbon export in the Sargasso Sea is countered by its enhanced mesopelagic attenuation" *by* M. W. Lomas et al.

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We thank for the reviewer for his/her valuable comments and the general statement regarding the overall sound analysis. Below we have addressed all of the major and minor comments. Where we don't necessarily agree with reviewer we provide our rationale. We agree with the reviewer that the title needs some work so it more accurately reflects the nature of the manuscript. We suggest the following new title. "Increased ocean carbon export in the Sargasso Sea linked to climate variability is countered by its enhanced mesopelagic attenuation"

Specific Comments:

1. Trap deployment and preservation. The trap deployments ranged 3-4 days and the traps contained a brine solution (50 g NaCl/L above ambient) with formaldehyde (0.7% v/v). This information has been added to the text.

2. Contribution of Synechococcus to increased Chla. The increase in absolute pigment biomass attributed to Synechococcus (bars in Figure 3C) is more 'apparent' than the increase in the relative contribution of Synechococcus to Chla. This might be an optical illusion given that the scale for the relative contribution is twice that of the absolute concentration. In addition, as the cell counts were done by flow cytometry we also have data on phycoerythrin and Chla per cell for Synechococcus. Our data suggest that the pigment fluorescence per cell has decreased substantially over time, but the decrease has been comparable for PE and chla. So the use of a fixed pigment ratio is still valid for the HPLC analysis, but it does create a disconnect between cell numbers and pigment biomass. This later point is a work in progress and isn't ready for prime time, however we have added a qualifying to statement to the text to address this important comment.

3. Changes in POC remineralization. Indeed, the figure reference should have been 4a and not 4d; this has been corrected. Regarding the different patterns in Teff and AOU, we agree this doesn't provide the most solid foundation, although there is value in the observation that the trend for both parameters is in a direction that results in a consistent interpretation. As requested above, we have added information on the trap deployment as well as a statement on the variable and significant dissolution of POC post-collection that can approach 30% for carbon (Antia 2005). The importance of this dissolution possible exhibits a trajectory as the phytoplankton community changes (as alluded to by the reviewer), with a larger dissolution, and therefore lower Teff, that is independent of heterotrophic metabolism (ie. AOU estimate). Unfortunately, we don't have data on this so can only discuss in generalities, but we have added some additional discussion text on this point. The modified paragraph reads as follows. "The absolute magnitude of POC attenuation in the mesopelagic zone (150-300 m) was

greater after ca. 1996 than in the prior decade (Student's t-test with unequal variance, $P < 0.01$; Fig. 4a). This increase in POC remineralization was associated with a significant (Student's t-test with unequal variance, $P < 0.01$; Fig. 4D) increase in apparent oxygen utilization (AOU; Fig. 4D) suggesting that POC attenuation may be due to increased mesopelagic metabolic activity (Steinberg et al., 2008b). The temporal patterns in Teff and AOU are different and could suggest other explanations. Substantial dissolution of particulate material post-collection in the trap tubes can occur averaging ∼30% for carbon (reviewed by Antia 2005). The reasonably monotonic trend in phytoplankton community structure increasingly dominated by small prokaryotes might result in the dissolution of an increasingly large fraction of the trap POC and therefore Teff, as it was calculated from trap measurements, might reflect dissolution as much as metabolic loss of POC. The observed step-change in AOU reflects complications in the calculation itself (see methods for details) as well as a more rapid change in system response. Reconciling the temporal response of these two estimates of mesopelagic carbon attenutation would require more focused process research, particularly focusing on the response of bacteria versus metazoans with time."

4. Zooplankton changes over time and production estimates. Re: changes over time, we have re-run the regression (figure 6) excluding all of the 2004 data, which do contribute substantially to the positive trend, and still find a significant ($P=0.019$) regression. Granted the slope of the regression is lower but it remains significant. More importantly, the wide range of data in 2004 appears to be specific to the 200-500um size class (the data presented). The total zooplankton biomass does not show that same disparity and indeed the increase for the whole is greater than for the specific size class shown. We have chosen to present the data we did for the simple reason that Debbie Steinberg is working on the BATS zooplankton timeseries and we do not wish to steal any of her thunder. That said we have added a paragraph to the revised text that makes the very broad brush calculation suggested by this reviewer What it shows is that within a factor of 2-4 the increase in zooplankton biomass over time can account for the change in AOU. The 'shapes' of the temporal patterns remain differ-

ent (the zooplankton show a steady increase with time as does the decrease in Teff) whereas AOU is more of a square wave (see previous response). The new paragraph is as follows. "...At this time there is no data to directly evaluate the former, although it is hypothesized that attached bacterial activity would increase with increased POC flux. Total epipelagic mesozooplankton biomass in the Sargasso Sea, here only the data for the 200-500 μm size class is shown but it is representative of data in the other size classes, has increased approximately 2.5 fold since 1994 (Steinberg et al., 2008a; Fig. 6). Too few data are available prior to 1994 to determine if there was a different trend in zooplankton biomass in the early 1990's. As mesozooplankton metabolic rates and production generally scale with biomass (e.g., Roman et al., 2002), the increased mesozooplankton biomass could equate to increased metabolic demand and support some of the observed increases in AOU. From data presented in Roman et al. (2002), an average (1994-1997) total mesozooplankton respiration rate (and therefore approximate contribution to AOU) at BATS, integrated from 0-150m, of $\sim$2.2 mmol C m-2 d-1 can be calculated; a value slightly larger than the ΔPOC observed during the same period (Fig. 4). Using the period of overlap between the data of Roman et al. (2002) and Steinberg et al. (2008), 1994-1997, to a first approximation, the longer dry weight biomass record (Fig. 6) can be converted to a carbon biomass record. Assuming a constant growth rate with time (0.15 d-1 determined by Roman et al. 2002) and repeating the calculations, the increase in biomass would increase mesozooplankton respiration rates to $\sim$5.5 mmol C m-2 d-1 at the end of the time-series. This change in respiration over time, $\sim$3.3 mmol C m-2 d-1, compares quite favorably with the change in AOU before and after $\sim$1996, 4.1 mmol C m-2 d-1. As a worst case scenario, a 50% reduction in growth rate would still result in a net increase of $\sim$1.2 mmol C m-2 d-1 in the latter decade associated with the known increase in biomass, although with an increase in food, one might hypothesize zooplankton growth rates would increase not decrease. Based on these back of the envelope calculations, it appears that the change in zooplankton biomass and resultant respiration may account for a significant fraction of the increase in AOU. It is important to point out however, that some of the attenuation of

POC flux may also be due to fragmentation of large aggregates into smaller particles with slower sinking rates by biological processes such as particle-attached microbial activity, zooplankton feeding, or zooplankton-induced shear (Steinberg et al., 2008b). "

5. Inverse relationship between MLD coefficient of variation and biological carbon parameters. We think the reviewer might have misread Table 3. Actually many of the biological carbon pump parameters show significant negative correlations MLD-coefficient: PProd (-0.49, P=0.05), TChla (-0.48, P=0.05), POCflux (-0.65, P=0.01). We have added horizontal lines to Table 3 to ease in reading across rows. In addition, we've eliminated those entries that are not significant at the $P \leq 0.1$ level.

Technical Corrections:

1) PDO has been changed, thank you.

2) Corrected.

3) Dinoflagellates and Pelagophytes have been removed from Table2. Pigment based Prochlorococcus and direct cell count data have been left in as another reviewer asked if Prochlorococcus had increased. The data are interesting to have.

4) Corrected

5) Corrected

6) Primary production incubation volumes are 250ml – this has been added.

7) Yes, it should be density horizon, thank you for catching this.

8) The following sentence has been added at the beginning of the paragraph to help eliminate this confusion. "The data presented in this manuscript are only from the winter/spring period each year and therefore no seasonal detrended of the data was done. "

9) Yes, the correlations in Table 2 are correct. 'n' does equal 'N' and these was a

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carryover error from several iterations of the manuscript. This has now been corrected.

10) Figure 5 has been removed and all others renumbered.

11) Corrected

12) This sentence has been rewritten as: "Total epipelagic mesozooplankton biomass in the Sargasso Sea (e.g., 200-500 μm size class), has increased approximately 2.5 fold since 1994 (Steinberg et al., 2008a; Fig. 5)."

13) This sentence has been deleted and the numerical values moved forward in the text so it is only stated once.

14) Corrected, thank you.

15) This has been clarified.

16) Lines have been removed from this figure and the units have been corrected.

17) Figure 5 has been deleted.

18) We have modified the figure caption for Figure 6 (now Figure 5). We have chosen to leave the white space in so that when readers look at figures with an X-axis of 'year' they will be looking at the same scale. This may help in visualizing the timing of changes at BATS.

19) The data in Figure 8 (now Figure 7) are those used in the determining the correlation parameter in Table 3 (now Table 2). While we can see the reviewers point, just listing the correlation values in the table can be misleading as the reader doesn't get a sense of the distribution of data. Figure 8 (now figure 7) shows the data distribution and clearly shows that there is an even data distribution which we feel contributes to the overall 'strength' of the presentation.

Interactive comment on Biogeosciences Discuss., 6, 9547, 2009.